

## NATIVISM REVISITED

*A Review of Eric H. Lenneberg's Biological Foundations of Language*<sup>1</sup>

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In 1957, B. F. Skinner published his account of how a behavioral functional analysis might be extended to the domain of verbal behavior. Although the point was missed by some critics of his *Verbal Behavior*, Skinner did not claim to have actually performed the kind of detailed analysis outlined. As he stated in the opening pages: "The emphasis is upon an orderly arrangement of well-known facts, in accordance with a formulation of behavior derived from an experimental analysis of a more rigorous sort. The present extension to verbal behavior is thus an exercise in interpretation rather than a quantitative extrapolation of rigorous experimental results" [p. 11].

Lenneberg's *Biological Foundations of Language* is a similar document: "My theory of language development is essentially an interpretive commentary on observable facts. . . . Most of its tenets are merely special instances of the general premises [listed earlier] . . . and may, therefore, be considered as fairly common biological phenomena. . . . This book must be understood as a discussion rather than a presentation of the biological foundations of language. The exact foundations are still largely unknown" [p. viii and p. 379]. Both works, in short, constitute plausibility arguments for particular views of verbal behavior. They specifically address the same phenomena when they attempt to account for an individual's ability to produce and understand novel sentences in his native language.

But there the similarity ends. Skinner's approach epitomizes the Empiricist Tradition in which most American psychologists have worked; Lenneberg's represents a Nativist Tradition which has only recently been reinvigorated. We wish to emphasize that this difference is profound and pervasive, because it is often tempting to try to reconcile differing theoretical accounts by claiming that they merely reflect different levels of analysis or deal with different domains of phenomena. In fact, it would be easy to suppose that such is the case here.

Thus, Lenneberg is concerned with the biological bases of linguistic "competence". Unlike Skinner, he is not concerned with the controlling environmental variables which influence the emission probabilities of verbal operants, but with the human organism's underlying capacity for linguistic performance. Furthermore, he does not deny that children "learn" their native languages from their social environments in some way nor that the empirical Law of Effect might play some role in the maintenance, if not the acquisition, of verbal behavior, perhaps even in ways similar to those described by Skinner in the first two-thirds of *Verbal Behavior*. For his part, Skinner has often pointed out that biological considerations might well illuminate the functional relations that have emerged from his analyses, and if others so inclined can trace the phylogenetic antecedents and physiological bases of these relations, so much the better for our understanding of human behavior. Skinner correctly holds that such additional knowledge can neither disprove those functional relations nor displace them as independent scientific contributions. Finally, behaviorists are themselves "nativists" in pursuing their working assumptions that experimental organisms come "pre-wired" so as to be susceptible to operant and respondent con-

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ditioning; that human organisms, in particular, are inherently capable of learning subtle discriminations among linguistic stimuli; and that the human brain is sufficiently complex and well organized to enable even young children to generalize from learned linguistic utterances to novel ones which are similar. (About which, more later.) Thus, it would seem that both Lenneberg and Skinner could lay equal claim to our credibility with their "plausibility arguments".

But this apparent peaceful coexistence of the two approaches is only superficial; the different-levels-of-analysis gambit for reconciliation yields an illusory consensus that could not be sustained as the domains of inquiry continue to converge, as they inevitably will. In areas where the two analyses already overlap (e.g., first-language acquisition and the ability to manipulate syntactic relations), it is abundantly clear that the plausibility of one of them necessarily precludes the plausibility of the other. The two interpretations of "well-known facts" disagree at all levels from the basic philosophical level about the nature of man, through differing beliefs about what the relevant well-known facts really are, to the tiniest detail of why a child will say "Johnny runned".

None of this is incidental. The decade separating the publication of these two books spans a period of profound revolution in linguistic thought, a revolution fomented almost entirely by Noam Chomsky of MIT, whose theory of "transformational grammar" is still highly controversial within linguistics itself. Chomsky's ideas were first seen by psychologists in 1959, when he wrote his now-classic, 32-page review of Skinner's *Verbal Behavior*. But many students of verbal behavior who disagreed with Skinner's formulation chuckled over Chomsky's elegant rhetorical style without appreciating the fact that the review constituted a basic attack upon the very Empiricist Tradition in which they themselves were working. That fact has become much more widely appreciated now that more and more psychologists have participated in painful "cross-fertilization" conferences with the new linguists, only to expose their own efforts to the same kinds of criticism, couched in the same flashy rhetorical style which has apparently become a *sine qua non* for admission to the ranks of the new

linguists. Years of psychological research and arduously constructed psychological theories in the behavioral tradition are, in the linguist's view (and style), plainly irrelevant to any nontrivial problem of language which could be of any conceivable interest—hopelessly inadequate, *in principle*, to deal with the most elementary facts of linguistic competence, as one can clearly see if the topic is approached in anything like a serious way—obviously absurd if taken literally, and, if interpreted metaphorically, serving only to obscure important linguistic distinctions which are apprehended by any 18-month-old child who has had a few hours of exposure to his native language. Even complex multistage mediational models of language learning are gleefully demonstrated to require the child to learn the values of  $10^9$  parameters in a childhood lasting only  $10^8$  seconds.

The persuasive force of the new linguists, however, derives not from their debating skills, which are considerable, but from the success of their formal analyses of linguistic data. These analyses represent a significant achievement which evokes admiration whether or not one agrees with the psychological claims about language that are alleged to flow from them. (Chomsky provides a brief but excellent overview of these analyses in an appendix to Lenneberg's book.) Accordingly, it is not biological considerations which are prompting an increasing number of psychologists to accept some of the extraordinary conclusions about the innate character of linguistic competence, but the apparent failure of any current notions about learning to account for the new linguistic observations in even a remotely satisfying way. Some well-known psychologists (e.g., Jenkins, 1968) have made public "*mea culpa's*" as mediational models of language have withered under the attack, while others, more discreetly, are privately following suit. And, even though one of us has found Skinner's approach to verbal behavior heuristically valuable for illuminating certain non-linguistic phenomena (Bem, 1965), we, too, are among the persuaded.

Anyone unaware of this bit of contemporary history is likely to miss an important mission of Lenneberg's book: to provide a palpable biological plausibility for conclusions to which a number of uncomfortable

Empiricists have recently committed themselves on the basis of formal argument alone. This mission is not obvious from Lenneberg's ordering or treatment of topics, from its "surface structure" as the new linguists would say.

Thus, the first six chapters begin with a quiet orderly exposition of the morphological, physiological, neurological, and genetic aspects of language insofar as they are known or can be guessed at. In Chapter Seven, "Primitive Stages in Language Development", Lenneberg alludes briefly to the formal arguments of the linguists in defining the nature of the problem and the framework to be employed. He then goes on to an excellent review of recent work on children's linguistic development, greatly enriching his account with his own findings on speech pathology in defective children. This chapter is by far the strongest in the book and lends persuasive empirical substance to the psychological speculations of the new linguists. In fact, this chapter, in conjunction with Chomsky's appendix, comprises the best available introduction to the "new look" in language for anyone who has not yet encountered it; furthermore, it can be read without reference to other materials in the book.

In Chapter Eight, "Language and Cognition", Lenneberg attempts to argue that linguistic competence is part of a more general cognitive competence. (He here parts company with many of the new linguists.) Although this chapter is suggestive, it is very speculative and serves primarily to illustrate the current state of ignorance by pointing up the difficulties that appear when one seriously approaches the topic of semantics, even at its most concrete levels. In contrast with the rest of the book, this chapter is relatively weak in wedding the empirical evidence to the conceptual thesis. As such, the chapter is almost empirically empty and is therefore subject to the same kinds of criticism from the behaviorists that Skinner's *Verbal Behavior* suffered at the hands of Chomsky. Nevertheless, the chapter is important because psychological research in cognitive processes will almost certainly be moving in this direction now that the linguists have provided a viable example of the approach. But, Chomsky's discussion of Universal Semantics in his appendix reveals that the linguists know no more than Lenneberg about this murky area.

In sum, Lenneberg's presentation is persuasive, and the weaker spots (e.g., Chapter Eight or his account of language change) do not impair the thrust of his major thesis. Because the actual knowledge of the biological foundations is very meager, the method of argument is often indirect and sometimes even negative (e.g., there is no positive evidence that reinforcement facilitates first-language learning). But Lenneberg is very adept at marshaling data from a wide variety of settings, and his use of findings on language pathology is particularly illuminating throughout. The path of acceptance for his Nativist thesis has also been eased somewhat by recent findings in perception, by the increased visibility of ethology, and even by the amusing difficulties of Breland and Breland (1961) in trying to keep operant conditioning free from entanglement with innate behavior patterns. "Wolf children", be it noted in passing, receive only one page, a healthy index of an advance in sophistication if not in knowledge.

Here then is about the best empirical foundation one could hope for at the present time for supporting the factual objections which the new linguists have raised to Empiricist accounts of linguistic competence: the ability to learn language appears to be species-specific, to possess a critical period, to develop independently of general intelligence, to have an orderly chronological development not easily attributable to concomitant changes in the reinforcing environment, and to show peculiar pathologies whose character strongly suggests that linguistic competence resembles other ethological phenomena more closely than it resembles any kind of operant learning. Furthermore, natural languages resemble one another in surprising ways that are not easily attributable to similarities in the cultural pressures operating on historically unrelated languages.

But as we suggested earlier, these purely factual arguments do not comprise the Nativist's secret weapons. Indeed, they could tolerate being wrong on every one of these counts. To plagiarize from one of the wittiest: "... let us suppose, first, that the ability to learn a language like English turns out not to be species-specific. To make it interesting, let us imagine that some graduate student at Harvard develops a schedule of reinforce-

ment that permits us to teach English to worms. Second, we may suppose that it turns out that there are no important linguistic universals. . . . Finally, let us suppose that explicit reinforcement turns out to play a very much larger role in language learning than we now have any evidence for believing. A graduate student at Stanford discovers that 93.6% of all mothers queried ply their children with chicken soup when and only when they utter sentences in their native language. The point of all this is that the failure of all the empirical arguments thus far adduced would, quite certainly, leave the linguist's view of Empiricistic accounts of language assimilation substantially unaltered" (Fodor, 1967).

The Nativists will bring out their big guns when an Empiricist suggests that individuals are able to produce and understand novel sentences because of stimulus generalization from grammatical sentences already learned. Such an assertion immediately provokes questions about the nature of the dimension along which generalization takes place. That is, what properties do well-formed sentences possess in common that non-sentences do not? How does a child learn that one sentence is an exact paraphrase of another one with a different grammatical structure, even if he has not encountered it previously? To call the process "stimulus generalization" is merely to label the problem (albeit awkwardly, the linguists will claim), not to provide an answer. It is an empty gesture until one can specify the nature of the similarity involved and how the child came to respond to that.

The linguists can then provide a series of examples which easily confound any current or foreseeable version of theories based upon the learning of grammatical frames, mediating mechanisms (no matter how multileveled and elaborate), or autoclitic processes (Skinner, 1957). To select from their popular stock of such examples: how does a young child learn that "I expected the doctor to examine John" is the same as "I expected John to be examined by the doctor", while at the same time learning that "I persuaded the doctor to examine John" is not the same as "I persuaded John to be examined by the doctor"? How do we learn to respond correctly to the grammatical relations that differentiate "John is eager to please", "John is easy to please",

"John is eager to eat", "John is easy to eat", "John is too eager to eat", "John is too eager to please", *etc. etc.* Or, finally, why would paraphrases of the following identical frames be so different? "They are drinking high-balls", "They are drinking glasses", "They are drinking companions".

So far, no learning-theoretic set of hypotheses has come even close to answering questions like these. Thus, the linguists are able to argue that the examples can be plausibly accounted for only by supposing that they are connected at a very deep and abstract level, and that the human organism must necessarily be "pre-wired" to deal with the transformational rules that specify the necessary interrelations. The persuasiveness of their argument lies in their concrete accomplishment, namely, the formal apparatus that spells out in some detail the "dimension of stimulus generalization" in a nontrivial way, at least in principle.

Except in their unguarded moments, the linguists do not equate their formal model with a psychological theory *per se*, but they do maintain that any psychological model of linguistic *performance* must necessarily incorporate their formal model of linguistic *competence*, or something very much like it. Their Nativist position will hold the fort until some learning theory can either define the syntactical relations with anything simpler than a full transformational grammar and/or show "how it got there". That day may come, but it seems unlikely that the successful theory will look much like a simple extension of any current Empiricistic notions of "what is learned" and how. When linguists assert that current behavioral theories are inadequate *in principle*, it is tempting to allude to previous formal proofs that bumblebees could not possibly fly. The sober fact is, however, that no bumblebee could fly if it adhered to aerodynamic principles of flight that apply to other insects but that fail him because of his species-specific construction.

That, in short, is the secret weapon of the linguists. And that is why Empiricists are becoming Nativists in these latter days.

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